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1st.December 1955.

Professor J.Lederberg,
Department of Genetics,
University of Wisconsin,
Madison,
Wis, U.S.A.

Dear Josh,

Thanks for your letter of Nov.25th. I was sorry
to hear about your father's illness, and about your other
difficulties. I hope that things are now coming more under control.
I am glad to hear that your lab. is being re-modelled, but I
can imagine the disorganisation the move must have caused you all.

As to paper:-

(1) Your draft. I thought I had sent you my immediate reactions:
I will go over it again and send you a more detailed opinion.
One difficulty about expressing an opinion, especially as to the
points on which we differ, was the absence of tables and pedigrees
(I appreciate the difficulty of getting these into suitable shape,
having had much trouble that way myself. In general there are
3 main differences in our drafts.

(i) Different style. Your presentation is much more compressed,
mine, in my efforts to be comprehensible even by the ill-informed,
possibly goes too far the other way.

(ii) You have worked out more or less all possible hypotheses,
whereas I have stated only what I consider to be the simplest
hypothesis which will account for my observations.

(iii) Different results and different interpretation. I think
these result from our use of SW 541 and SW 666 respectively, and
to a less extent from minor differences in methods. I have done
some work with SW 666, and find that it gives results which while
compatible with my hypothesis might perhaps be explained on other
hypotheses, which is not I think the case with my SW 541 data,
broadly speaking.

As to question of joint or separate papers,

(i)   difference in style etc would, at this stage anyway, cause
considerable ~~difficulty~~ *(shown)* in arriving at a draft acceptable to
us both.

(ii) Your more complete theoretical analysis could of course be
worked into a joint paper; but in a separate paper would complement
my presentation, in a way that would be useful to me for cross-
reference. *ie. support my conclusions*

(iii) This is the real difficulty. I feel pretty confident that
my data adequately support my conclusions; your data do not do
so. If we do a joint paper now, I should either have to be much
less definite in my conclusions, or you would have to commit
yourself to conclusions which your own results did not, by
themselves, establish. You, I take it, would be reluctant to do
this and I am reluctant to weaken my conclusions, partly because
I think they are O.K. and partly the considerable difficulty of
stating them in an understandable and convincing way would be
much increased if one had to sit on the fence as to their validity.

For these reasons I think it would probably be a mistake
to change again now and try to do a joint paper. I see no
objection to separate ones, except that people may be confused to
find that our conclusions (to some extent) differ; but as they do
differ (at the moment) not much would be gained by concealing this
in a joint paper.

As to separate papers together or apart, I am inclined to
favour apart; for various reasons, particularly audience reached
and availability of space etc. I favour the J.G.M. But I
agree something aimed at the geneticists would be desirable and
I think there is a lot to be said for me aiming at one audience
and you for another,

I feel sure you won't mind this. One further reason for
separate presentation, which I would not allow to count by itself,
is that I am well on with re-writing my present draft, and would
hate to have to scrap it and start again; the more so as I am
pre-occupied with various time-using pursuits, including helping
to edit next years Soc.gen.Microbiol.Symposium volume (Bacterial
Anatomy). The one thing I regret is that its not possible for us
to work in the same lab. on it for 2 or 3 weeks, since this would
probably resolve our differences of interpretation etc. (Even an
hour or two or argument might help quite a bit). I assume that
we don't have to worry about which paper comes out first; anyway

we should both be stating that we had collaborated so far as possible.

That seems to be about all there is to say on the paper question.

I remembered after the letter was posted that I had omitted a figure from the sentence about discriminating E from non-E. The figure I have in draft is 15 (I had forgotten it).

Another point I forgot last ~~night~~ time. I have written a thing about flagella for this symposium of the S.g.M., and have a short section on unilinear transmission of mcp, using Quadling's data mostly; but I quoted you (L; pers.comm.) for your 60 generation example. Is this O.K.? Sorry I forgot to ask you before. If its not O.K. it can come out in proof, but as it has got to proof stage now I would like to know by return if its not O.K.

I gave your message to Felix. I have not discussed the Vi transfer story, which sounded odd. (So did all that stuff about B.anthracis in J.Bact. not long back; I hear some of their 'transferred' strains behave as B.cereus and others as B.subtilis, but don't quote this as I have not details).

Yours sincerely,

I think its your sign "weakly, as usual" that creates the impression.

B.Stocker.

I am sorry to hear you sounding so gloomy about lab and (it reads) about life. I suspect you need a holiday. I think you should be given a year off, for a trip around. I hope the house is going well.

Hope to hear again if you have time; meantime I shall, as opportunity arises, press on with my own draft, & will send you a carbon as soon as its done (I don't know how long this will be.

With Madame de Marquise's wild I am just starting down new expt. on SW666, as preliminaries to serum immob. test. On re-consideration I don't think I can recognise an E cell by inspection even in SW541, or rather but regularly, so am not hopeful of doing so in SW666. My love to Esther, Yrs Bruce